ON SENSATIONS OF ORIENTATION.[1]

THROUGH the co-operation of a succession of inquirers, among whom are particularly to be mentioned Goltz of Strassburg and Breuer of Vienna, considerable advances have been made during the last twenty-five years in our knowledge of the means by which we ascertain our position in space and the direction of our motion, or orient ourselves, as the phrase goes. I presume that you are already acquainted with the physiological part of the processes with which our sensations of movement, or, more generally speaking, our sensations of orientation, are connected. Here I shall consider more particularly the physical side of the matter. In fact, I was originally led to the consideration of these questions by the observation of extremely simple and perfectly well-known physical facts, before I had any great acquaintance with physiology and while pursuing unbiasedly my natural thoughts; and I am of the conviction that the way which I have pursued, and which is entirely free from hypotheses, will, if you will follow my exposition, be that of easiest acquisition for the most of you.

No man of sound common sense could ever have doubted that a pressure or force is requisite to set a body in motion in a given direction and that a contrary pressure is required to stop suddenly a body in motion. Though the law of inertia was first formulated with anything like exactness by Galileo, the facts at the basis of it were known long previously to men of the stamp of Leonardo da

[1] A lecture delivered on February 24, 1897, before the *Verein zur Verbreitung naturwissenschaftlicher Kenntnisse in Wien*. Translated by T. J. McCormack.

Vinci, Rabelais, and others, and were illustrated by them with appropriate experiments. Leonardo knew that by a swift stroke with a ruler one can knock out from a vertical column of checkers a single checker without overthrowing the column. The experiment with a coin resting on a piece of pasteboard covering a goblet, which falls into the goblet when the pasteboard is jerked away, like all experiments of the kind, is certainly very old.

With Galileo the experience in question assumes greater clearness and force. In the famous dialogue on the Copernican system which cost him his freedom, he explains the tides in an infelicitous, though in principle correct manner, by the analogue of a platter of water swung to and fro. In opposition to the Aristotelians of his time, who believed the descent of a heavy body could be accelerated by the superposition of another heavy body, he asserted that a body could never be accelerated by one lying upon it unless the first in some way impeded the superposed body in its descent. To seek to press a falling body by means of another placed upon it, is as senseless as trying to prod a man with a lance when the man is speeding away from one with the same velocity as the lance. Even this little excursion into physics can explain much to us. You know the peculiar sensation which one has in falling, as when one jumps from a high springboard into the water, and which is also experienced in some measure at the beginning of the descent of elevators and swings. The reciprocal gravitational pressure of the different parts of our body, which is certainly felt in some manner, vanishes in free descent, or, in the case of the elevator, is diminished on the beginning of the descent. A similar sensation would be experienced if we were suddenly transported to the moon where the acceleration of gravity is much less than upon the earth. I was led to these considerations in 1866 by a suggestion in physics, and having also taken into account the alterations of the blood-pressure in the cases in question, I found I coincided without knowing it with Wollaston and Purkinje. The first as early as 1810 in his Croonian lecture had touched on the subject of sea-sickness and explained it by alterations of the blood-

pressure, and later had laid similar considerations at the basis of his explanation of vertigo (1820–1826).[1]

Newton was the first to enunciate with perfect generality that a body can change the velocity and direction of its motion only by the action of a force, or the action of a second body. A corollary of this law which was first expressly deduced by Euler is that a body can never be set *rotating* or made to cease rotating of itself but only by forces and other bodies. For example, turn an open watch which has run down freely backwards and forwards in your hand. The balance-wheel will not fully catch the rapid rotations, it does not even respond fully to the elastic force of the spring which proves too weak to carry the wheel entirely with it.

Let us consider now that whether we move ourselves by means of our legs, or whether we are moved by a vehicle or a boat, at first only a part of our body is directly moved and the rest of it is afterwards set in motion by the first part. We see that pressures, pulls, and tensions are always produced between the parts of the body in this action, which pressures, pulls, and tensions give rise to sensations by which the forward or rotary movements in which we are engaged are made perceptible. But it is quite natural that sensations so familiar should be little noticed and that attention should be drawn to them only under special circumstances when they occur unexpectedly or with unusual strength.

Thus my attention was drawn to this point by the sensation of falling and subsequently by another singular occurrence. I was rounding a sharp railway curve once when I suddenly saw all the trees, houses, and factory chimneys along the track swerve from the vertical and assume a strikingly inclined position. What had hitherto appeared to me perfectly natural, namely, the fact that we distinguish the vertical so perfectly and sharply from every other direction, now struck me as enigmatical. Why is it that the same direction can now appear vertical to me and now

---

[1] Wollaston, *Philosophical Transactions, Royal Society*, 1810. In the same place Wollaston also describes and explains the creaking of the muscles. My attention was recently called to this work by Dr. W. Pascheles.—Cf. also Purkinje, *Prager medicin. Jahrbücher*, Bd. 6, Wien, 1820.

cannot? By what is the vertical distinguished for us? (Compare Figure 1.)

The rails are raised on the convex or outward side of the track in order to insure the stability of the carriage as against the action of the centrifugal force, the whole being so arranged that the combination of the force of gravity with the centrifugal force of the train shall give rise to a force perpendicular to the plane of the rails.

Let us assume, now, that under all circumstances we somehow sense the direction of the total resultant mass-acceleration whencesoever it may arise as the vertical. Then both the ordinary and the extraordinary phenomena will be alike rendered intelligible.

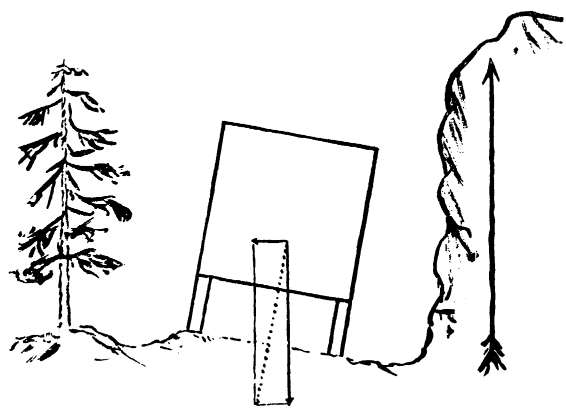

Fig. 1.

I was now desirous of putting the view I had reached to a more convenient and exact test than was possible on a railway journey where one has no control over the determining circumstances and cannot alter them at will. I accordingly had the simple apparatus constructed which is represented in Figure 2.

In a large frame *BB*, which is fastened to the walls, rotates about a vertical axis *AA* a second frame *RR*, and within the latter a third one *rr*, which can be set at any distance and position from the axis, made stationary or movable, and is provided with a chair for the observer.

The observer takes his seat in the chair and to prevent dis-

turbances of judgment is enclosed in a paper box. If the observer together with the frame *rr* be then set in uniform rotation, he will feel and see the beginning of the rotation both as to direction and amount very distinctly although every outward visible or tangible point of reference is wanting. If the motion be uniformly continued the sensation of rotation will gradually cease entirely and the observer will imagine himself at rest. But if *rr* be placed outside the axis of rotation, at once on the rotation beginning, a strikingly apparent, palpable, actually visible inclination of the entire paper box is produced, slight when the rotation is slow, strong when the rotation is rapid, and continuing as long as the rotation lasts. It is

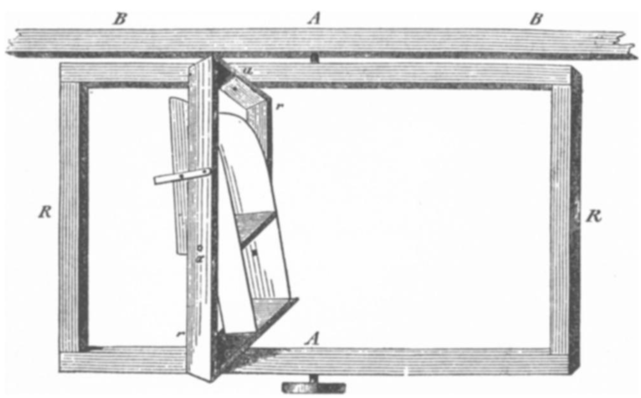

Fig. 2.
From Mach's *Bewegungsempfindungen*, Leipsic, Engelmann, 1875.

absolutely impossible for the observer to escape perceiving the inclination, although here also all outward points of reference are wanting. If the observer, for example, is seated so as to look towards the axis, he will feel the box strongly tipped backwards, as it necessarily must be if the direction of the total resultant force is perceived as the vertical. For other positions of the observer the situation is similar.[1]

Once, while performing one of these experiments, and after

[1] It will be observed that my way of thinking and experimenting here is related to that which led Knight to the discovery and investigation of the geotropism of plants, *Philosophical Transactions*, January 9, 1806. The relations between vegetable and animal geotropism have been more recently investigated by J. Loeb.

rotating so long that I was no longer conscious of the movement, I suddenly caused the apparatus to be stopped, whereupon I immediately felt and saw myself with the whole box rapidly flung round in rotation in the opposite direction, although I knew that the whole apparatus was at rest and every outward point of reference for the perception of motion was wanting. Every one who disbelieves in sensations of movement should be made acquainted with these phenomena. Had Newton known them and had he ever observed how we may actually imagine ourselves turned and displaced in space without the assistance of stationary bodies as points of reference, he would certainly have been confirmed more than ever in his unfortunate speculations regarding absolute space.

The sensation of rotation in the opposite direction after the apparatus has been stopped, slowly and gradually ceases. But on accidentally inclining my head once during this occurrence, the axis of apparent rotation was also observed to incline in exactly the same manner both as to direction and as to amount. It is accordingly clear that the acceleration or retardation of rotation is felt. The acceleration operates as a stimulus. The sensation, however, like almost all sensations, though it gradually decreases, lasts perceptibly longer than the stimulus. Hence the long continued apparent rotation after the stopping of the apparatus. The organ, however, which causes the persistence of this sensation must have its seat in the *head*, since otherwise the axis of apparent rotation could not assume the same motion as the head.

If I were to say, now, that a light had flashed upon me in making these last observations, the expression would be a feeble one. I ought to say, I experienced a perfect illumination. My juvenile experiences of vertigo occurred to me. I remembered Flourens's experiments relative to the section of the semi-circular canals of the labyrinths of doves and rabbits, where this inquirer had observed phenomena similar to vertigo, but which he preferred to interpret, from his bias to the acoustic theory of the labyrinth, as the expression of painful auditive disturbances. I saw that Goltz had nearly but not quite hit the bull's eye with his theory of the semi-circular canals. This inquirer, who, from his happy habit

of following his own natural thoughts without regard for tradition, has cleared up so much in science, spoke, as early as 1870, on the ground of experiments, as follows: "It is uncertain whether the semi-circular canals are auditive organs or not. In any event they form an apparatus which serves for the preservation of equilibrium. They are, so to speak, the sense-organs of equilibrium of the head and indirectly of the whole body." I remembered the galvanic dizziness which had been observed by Ritter and Purkinje on the passage of a current through the head, when the persons experimented upon imagined they were falling towards the cathode. The experiment was immediately repeated, and sometime later (1874) I was enabled to demonstrate the same objectively with fishes, all of which placed themselves sidewise and in the same direction in the field of the current as if at command.[1] Müller's doctrine of specific energies now appeared to me to bring all these new and old observations into a simple, connected unity.

Let us picture to ourselves the labyrinth of the ear with its three semi-circular canals lying in three mutually perpendicular planes (Cf. Fig. 3, p. 86), the mysterious position of which inquirers have endeavored to explain in every possible and impossible way. Let us conceive the nerves of the ampullæ, or the dilated extensions of the semi-circular canals, equipped with a capacity for responding to every imaginable stimulus with a sensation of rotation just as the nerves of the retina of the eye when excited by pressures, by electrical or chemical stimuli always respond with the sensation of light; let us picture to ourselves, further, that the usual excitation of the ampullæ nerves is produced by the inertia of the contents of the semi-circular canals, which contents on suitable rotations in the plane of the semi-circular canal are left behind in the motion, or at least have a tendency to remain behind and consequently exert a pressure. It will be seen that on this supposition all the single facts which without the theory appear as so many

[1] This experiment is doubtless related to the galvanotropic experiment with the larvæ of frogs described ten years later by L. Hermann. Compare on this point my remarks in the *Anzeiger der Wiener Akademie*, 1886, No. 21. Recent experiments in galvanotropism are due to J. Loeb.

different individual phenomena, become from this single point of view clear and intelligible.

I had the satisfaction, immediately after the communication in which I set forth this idea,[1] of seeing a paper by Breuer appear[2] in which this author had arrived by entirely different methods at results that agreed in all essential points with my own. A few weeks later appeared the researches of Crum Brown of Edinburgh, whose methods were even still nearer mine. Breuer's paper was far richer in physiological respects than mine, and he had particularly gone into greater detail in his investigation of the collateral effects of

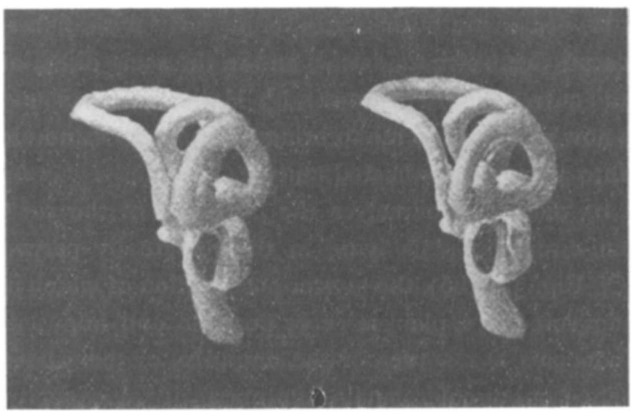

Fig. 3.
The labyrinth of a dove (stereoscopically reproduced), from R. Ewald, *Nervus Octavus*, Wiesbaden, Bergmann, 1892.

the reflex motions and orientation of the eyes in the phenomena under consideration.[3] In addition certain experiments which I had suggested in my paper as a test of the correctness of the view in question had already been performed by Breuer. Breuer has also rendered services of the highest order in the further elaboration of this field. But in a physical regard, my paper was, of course, more complete.

---

[1] *Wiener Akad.*, 6 November, 1873.

[2] *Wiener Gesellschaft der Aerzte*, 14 November, 1874.

[3] I have made a contribution to this last question in my *Analysis of the Sensations* (1886), English translation, 1897.

In order to portray to the eye the behavior of the semi-circular canals, I have constructed here a little apparatus. (See Fig. 4.) The large rotatable disc represents the osseous semi-circular canal, which is continuous with the bones of the head; the small disc, which is free to rotate on the axis of the first, represents the mobile and partly liquid contents of the semi-circular canal. On rotating the large disc, the small disc as you see remains behind. I have to turn some time before the small disc is carried along with the large one by friction. But if I now stop the large disc the small disc as you see continues to rotate.

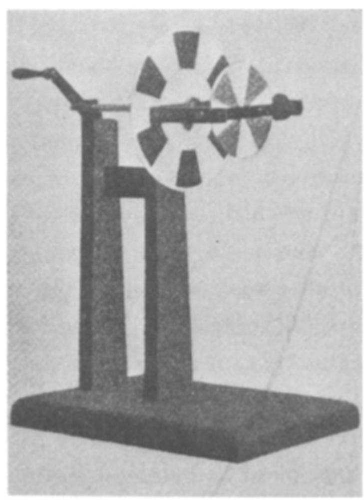

Fig. 4.
Model representing the action of the semi-circular canals.

Simply assume now that the rotation of the small disc, say in the direction of the hands of a watch, would give rise to a sensation of rotation in the opposite direction, and conversely, and you already understand a good portion of the facts above set forth. The explanation still holds, even if the small disc does not perform appreciable rotations but is checked by a contrivance similar to an elastic spring, the tension of which disengages a sensation. Conceive, now, three such contrivances with their mutually perpendicular planes of rotation joined together so as to form a single apparatus; then to this apparatus as a whole, no rotation can be imparted without its being indicated by the small mobile discs or by the springs which are attached to them. Conceive both the right and the left ear equipped with such an apparatus, and you will find that it answers all the purposes of the semi-circular canals, which you see represented stereoscopically in Fig. 3 for the ear of a dove.

Of the many experiments which I have made on my own person, and the results of which could be predicted by the new view

according to the behavior of the model and consequently according to the rules of mechanics, I shall cite but one. I fasten a horizontal board in the frame *RR* of my rotatory apparatus, lie down upon the same with my right ear upon the board, and cause the apparatus to be uniformly rotated. As soon as I no longer perceive the rotation, I turn around upon my left ear and immediately the sensation of rotation again starts up with marked vividness. The experiment can be repeated as often as one wishes. A slight turn of the head even is sufficient for reviving the sensation of rotation which in the perfectly quiescent state at once disappears altogether.

We will imitate the experiment on the model. I turn the large disc until finally the small disc is carried along with it. If, now, while the rotation continues uniform, I burn off a little thread which you see here, the small disc will be flipped round by a spring into its own plane 180°, so as now to present its opposite side to you, when the rotation at once begins in the opposite direction.

We have consequently a very simple means for determining whether one is actually the subject or not of uniform and imperceptible rotations. If the earth rotated much more rapidly than it really does, or if our semi-circular canals were much more sensitive, a Nansen sleeping at the North Pole would be waked by a sensation of rotation every time he turned over. Foucault's pendulum experiment as a demonstration of the earth's rotation would be superfluous under such circumstances. The only reason we cannot prove the rotation of the earth with the help of our model, lies in the small angular velocity of the earth and in the consequent liability to great experimental errors.[1]

Aristotle has said that "The sweetest of all things is knowledge." And he is right. But if you were to suppose that the *publication* of a new view were productive of unbounded sweetness, you would be mightily mistaken. No one disturbs his fellow-men with a new view unpunished. Nor should the fact be made a sub-

---

[1] In my *Grundlinien der Lehre von den Bewegsempfindungen*, 1875, the matter occupying lines 4 to 13 of page 20 from below, which rests on an error, is, as I have also elsewhere remarked, to be stricken out. For another experiment related to that of Foucault, compare my *Mechanics*, p. 303.

ject of reproach to these fellow-men. To presume to revolutionise the current way of thinking with regard to any question, is no pleasant task, and above all not an easy one. They who have advanced new views know best what serious difficulties stand in their way. With honest and praiseworthy zeal, men set to work in search of everything that does not suit with them. They seek to discover whether they cannot explain the facts better or as well, or approximately as well, by the traditional views. And that, too, is justified. But at times some extremely artless animadversions are heard that almost nonplus us. "If a sixth sense existed it could not fail to have been discovered thousands of years ago." Indeed! There was a time, then, when only seven planets could have existed! But I do not believe that any one will lay any weight on the philological question whether the set of phenomena which we have been considering should be called a sense. The phenomena will not disappear when the name disappears. It was further said to me that animals exist which have no labyrinth, but which can yet orientate themselves, and that consequently the labyrinth has nothing to do with orientation. We do not walk forsooth with our legs, because snakes propel themselves without them!

But if the promulgator of a new idea cannot hope for any great pleasure from its publication, yet the critical process which his views undergo is extremely helpful to the subject-matter of them. All the defects which necessarily adhere to the new view are gradually discovered and eliminated. Over-rating and exaggeration give way to more sober estimates. And so it came about that it was found unpermissible to attribute all functions of orientation exclusively to the labyrinth. In these critical labors Delage, Aubert, Breuer, Ewald, and others have rendered distinguished services. It can also not fail to happen that fresh facts become known in this process which could have been predicted by the new view, which actually were predicted in part, and which consequently furnish a support for the new view. Breuer and Ewald succeeded in electrically and mechanically exciting the labyrinth, and even single parts of the labyrinth, and thus in producing the movements that belong to such stimuli. It was shown that when the semi-circular

canals were absent vertigo could not be produced, when the entire labyrinth was removed the orientation of the head was no longer possible, that without the labyrinth galvanic vertigo could not be induced. I myself constructed as early as 1875 an apparatus for observing animals in rotation, which was subsequently reinvented in various forms and has since received the name of "cyclostat."[1] In experiments with the most varied kinds of animals it was shown that, for example, the larvæ of frogs are not subject to vertigo until their semi-circular canals which at the start are wanting are developed (K. Schäfer).

A large percentage of the deaf and dumb are afflicted with grave affections of the labyrinth. The American psychologist, William James, has made whirling experiments with many deaf and dumb subjects, and in a large number of them found that susceptibility to giddiness is wanting. He also found that many deaf and dumb people on being ducked under water, whereby they lose their weight and consequently have no longer the full assistance of their muscular sense, utterly lose their sense of position in space, do not know which is up and which is down, and are thrown into the greatest consternation,—results which do not occur in normal men. Such facts are convincing proof that we do not orientate ourselves entirely by means of the labyrinth, important as it is for us. Dr. Kreidl has made experiments similar to those of James and found that not only is vertigo absent in deaf and dumb people when whirled about, but that also the reflex movements of the eyes which are normally induced by the labyrinth are wanting. Finally, Dr. Pollak has found that galvanic vertigo does not exist in a large percentage of the deaf and dumb. Neither the jerking movements nor the uniform movements of the eyes were observed which normal human beings exhibit in the Ritter and Purkinje experiment.

After the physicist has arrived at the idea that the semi-circular canals are the organ of sensation of rotation or of angular acceleration, he is next constrained to ask for the organs that mediate the sensation of acceleration noticed in forward movements. In search-

---

[1] *Anzeiger der Wiener Akad.*, 30 December, 1875.

ing for an organ for this function, he of course is not apt to select one that stands in no anatomical and spatial relation with the semi-circular canals. And in addition there are physiological considerations to be weighed. The preconceived opinion once having been abandoned that the *entire* labyrinth is auditory in its function, there remains after the cochlea is reserved for sensations of tone and the semi-circular canals for the sensation of angular acceleration, the vestibule for the discharge of additional functions. The vestibule, particularly the part of it known as the sacculus, appeared to me, by reason of the so-called otoliths which it contains, eminently adapted for being the organ of sensation of forward acceleration or of the position of the head. In this conjecture I again closely coincided with Breuer.

That a sensation of position, of direction and amount of mass-acceleration exists, our experience in elevators as well as of movement in curved paths is sufficient proof. I have also attempted to produce and destroy suddenly great velocities of forward movement by means of various contrivances of which I shall mention only one here. If, while enclosed in the paper box of my large whirling apparatus at some distance from the axis, my body is in uniform rotation which I no longer feel, and I then loosen the connexions of the frame *rr* with *R* thus making the former moveable and I then suddenly stop the larger frame, my forward motion is abruptly impeded while the frame *rr* continues to rotate. I imagine now that I am speeding on in a straight line in a direction opposite to that of the checked motion. Unfortunately, for many reasons it cannot be proved convincingly that the organ in question has its seat in the head. According to the opinion of Delage, the labyrinth has nothing to do with this particular sensation of movement. Breuer, on the other hand, is of the opinion that the organ of forward movement in man is stunted and the persistence of the sensation in question is too brief to permit our instituting experiments as obvious as in the case of rotation. In fact, Crum Brown once observed while in an irritated condition peculiar vertigal phenomena in his own person, which were all satisfactorily explained by an abnormally long persistence of the sensation of ro-

tation, and I myself in an analogous case on the stopping of a railway train felt the apparent backward motion in striking intensity and for an unusual length of time.

There is no doubt whatever that we feel changes of vertical acceleration, and it will appear from the following extremely probable that the otoliths of the vestibule are the sense-organ for the *direction* of the mass-acceleration. It will then be incompatible with a really logical view to regard the latter as incapable of sensing horizontal accelerations.

In the lower animals the analogue of the labyrinth is shrunk to a little vesicle filled with a liquid and containing tiny crystals, auditive stones, or otoliths, of greater specific gravity, suspended on minute hairs. These crystals appear physically well adapted for indicating both the direction of gravity and the direction of incipient movements. That they discharge the former function, Delage was the first to convince himself by experiments with lower animals which on the removal of the otoliths utterly lost their bearings and could no longer regain their normal position. Loeb also found that fishes without labyrinths swim now on their bellies and now on their backs. But the most remarkable, most beautiful, and most convincing experiment is that which Dr. Kreidl instituted with crustaceans. According to Hensen, certain Crustacea on sloughing spontaneously introduce fine grains of sand as auditive stones into their otolith vesicle. At the ingenious suggestion of S. Exner, Dr. Kreidl costrained some of these animals to put up with iron filings (*ferrum limatum*). If the pole of an electro-magnet be then brought near the animal, it will at once turn its back away from the pole accompanying the movement with appropriate reflex motions of the eye the moment the current is closed, exactly as if gravity had been brought to bear upon the animal in the same direction as the magnetic force.[1] This, in fact, is what should be expected from the function ascribed to the otoliths. If the eyes be covered with asphalt varnish, and the auditive sacs removed, the

[1] The experiment was specially interesting for me as I had already attempted in 1874, although with very little confidence and without success, to excite electromagnetically my own labyrinth through which I had caused a current to pass.

crustaceans lose their sense of direction utterly, tumble head over heels, lie on their side or their back indifferently. This does not happen when the eyes only are covered. For vertebrates, Breuer has demonstrated by searching investigations that the otoliths, or better, statoliths, slide in three planes parallel to the planes of the semi-circular canals, and are consequently perfectly adapted for indicating changes both in the amount and the direction of the mass-acceleration.[1]

I have already remarked that not every function of orientation can be ascribed exclusively to the labyrinth. The deaf and dumb who have to be immersed in water, and the crustaceans who must have their eyes closed if they are to be perfectly disorientated, are proof of this fact. I saw a blind cat at Hering's laboratory which to one who was not a very attentive observer behaved exactly like a seeing cat. It played nimbly with objects rolling on the floor, stuck its head inquisitively into open drawers, sprang dexterously upon chairs, ran with perfect accuracy through open doors, and never bumped against closed ones. The visual sense had here been rapidly replaced by the tactual and auditive senses. And it appears from Ewald's investigations that even after the labyrinths have been removed, animals gradually learn to move about again quite in the normal fashion, presumably because the eliminated function of the labyrinth is now performed by some part of the brain. A certain peculiar weakness of the muscles alone is perceptible which Ewald ascribes to the absence of the stimulus which is otherwise constantly emitted by the labyrinth (the labyrinth-tonus). But if the part of the brain which discharges the deputed function

---

[1] Perhaps the discussion concerning the peculiarity of cats always falling on their feet, which occupied the Parisian Academy, and, incidentally, Parisian society a few years ago, will be remembered here. I believe that the questions which arose are disposed of by the consideration advanced in my *Bewegungsempfindungen* (1875). I also partly gave, as early as 1866, the apparatus conceived by the Parisian scientists to illustrate the phenomena in question, One difficulty was left untouched in the Parisian debate. The otolith apparatus of the cat can render it no service in *free* descent. The cat, however, while at rest, doubtless knows its position in space and is instinctively conscious of the amount of movement which will put it on its feet,

be removed, the animals are again completely disorientated and absolutely helpless.

It may be said that the views enunciated by Breuer, Crum Brown and myself in 1873 and 1874, and which are substantially a fuller and richer development of Goltz's idea, have upon the whole been substantiated. In any event, they have exercised a helpful and stimulative influence. New problems have of course arisen in the course of the investigation which still await solution, and much work remains to be done. At the same time we see how fruitful the renewed co-operation of the various special departments of science may become after a period of isolation and invigorating labor apart.

I may be permitted, therefore, to consider the relation between hearing and orientation from another and more general point of view. What we call the auditive organ is in the lower animals simply a sac containing auditive stones. As we ascend the scale, 1, 2, 3 semi-circular canals gradually develop from them, whilst the structure of the otolith organ itself becomes more complicated. Finally, in the higher vertebrates, and particularly in the mammals, a part of the latter organ (the lagena) becomes the cochlea, which Helmholtz explained as the organ for sensations of tone. In the belief that the entire labyrinth was an auditive organ, Helmholtz, contrary to the results of his own masterly analysis, originally sought to interpret another part of the labyrinth as the organ of noises. I showed a long time ago (1873) that every tonal stimulus by shortening the duration of the excitation to a few vibrations, gradually loses its character of pitch and takes on that of a sharp, dry report or noise.[1] All the intervening stages between tones and noises can be exhibited. Such being the case, it will hardly be assumed that one organ is suddenly and at some given point replaced in function by another. On the basis of different experiments and reasonings S. Exner also regards the assumption of a special organ for the sensing of noises as unnecessary.

[1] See the Appendix to the English edition of my *Analysis of the Sensations*, Chicago, 1897.

If we will but reflect how small a portion of the labyrinth of higher animals is apparently in the service of the sense of hearing, and how large, on the other hand, the portion is which very likely serves the purposes of orientation, how much the first anatomical beginnings of the auditive sac of lower animals resemble that part of the fully developed labyrinth which does not hear, the view is irresistibly suggested which Breuer and I (1874, 1875) expressed, that the auditive organ took its development from an organ for sensing movements by adaptation to weak periodic motional stimuli, and that many apparatuses in the lower animals which are held to be organs of hearing are not auditive organs at all.[1]

This view appears to be perceptibly gaining ground. Dr. Kreidl by skilfully-planned experiments has arrived at the conclusion that even fishes do not hear, whereas E. H. Weber, in his day, regarded the ossicles which unite the air-bladder of fishes with the labyrinth as organs expressly designed for conducting sound from the former to the latter.[2] Störensen has investigated the excitation of sounds by the air-bladder of fishes, as also the conduction of shocks through Weber's ossicles. He regards the air-bladder as particularly adapted for receiving the noises made by other fishes and conducting them to the labyrinth. He has heard the loud grunting tones of the fishes in South American rivers, and is of the opinion that they allure and find each other in this manner. According to these views certain fishes are neither deaf nor dumb.[3] The question here involved might be solved perhaps by sharply distinguishing between the sensation of hearing proper, and the perception of shocks. The first-mentioned sensation may, even in the case of many vertebrates, be extremely restricted, or perhaps even absolutely wanting. But besides the auditive function, Weber's ossicles may perfectly well discharge some other function. Although, as Moreau has shown, the air-bladder itself is not an organ of equilibrium in the simple physical sense of Borelli, yet

---

[1] Compare my *Analysis of the Sensations*, p. 123 ff.

[2] E. H. Weber, *De aure et auditu hominis et animalium*, Lipsiae, 1820.

[3] Störensen, *Journ. Anat. Phys.*, London, Vol. 29 (1895).

doubtless some function of this character is still reserved for it. The union with the labyrinth favors this conception, and so a host of new problems rises here before us.

I should like to close with a reminiscence from the year 1863. Helmholtz's *Sensations of Tone* had just been published and the function of the cochlea now appeared clear to the whole world. In a private conversation which I had with a physician, the latter declared it to be an almost hopeless undertaking to seek to fathom the function of the other parts of the labyrinth, whereas I in youthful boldness maintained that the question could hardly fail to be solved, and that very soon, although of course I had then no glimmering of how it was to be done. Ten years later the question was substantially solved.

To-day, after having tried my powers frequently and in vain on many questions, I no longer believe that we can make short work of the problems of science. Nevertheless, I should not consider an "ignorabimus" as an expression of modesty, but rather as the opposite. That expression is a suitable one only with regard to problems which are wrongly formulated and which are therefore not problems at all. Every real problem can and will be solved in due course of time without supernatural divination, entirely by accurate observation and close, searching thought.

ERNST MACH.

VIENNA, AUSTRIA.